Comment on "The Labelling Theory of Mental Illness"
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COMMENT ON "THE LABELLING THEORY OF MENTAL ILLNESS"*

(COMMENT ON SCHEFF, ASR JUNE, 1974)

Although Scheff (1974) never specifically defines the "labelling theory of mental illness" that he is defending in his critique of labelling critiques, I assume he is referring specifically to his own presentation (Scheff, 1966) rather than to the more general statements of Becker, Erickson, Kitsuse and others who are or were of the persuasion. It may be useful, then, to present the nine propositions that make up Scheff's theory.

1. Residual rule-breaking arises from fundamentally diverse sources (1966:40).
2. Relative to the rate of treated mental illness, the rate of unrecorded residual rule-breaking is extremely high (1966:47).
3. Most residual rule-breaking is "denied" and is of transitory significance (1966:51).
4. Stereotyped imagery of mental disorder is learned in early childhood (1966:64).
5. The stereotypes of insanity are continually reaffirmed, inadvertently, in ordinary social interaction (1966:67).
6. Labelled deviants may be rewarded for playing the stereotyped deviant role (1966:84).
7. Labelled deviants are punished when they attempt to return to conventional roles (1966:87).
8. In a crisis occurring when a residual rule-breaker is publicly labelled, the deviant is highly suggestible and may accept the proffered role of the insane as the only alternative (1966:88).
9. Among residual rule-breakers, labelling is the single most important cause of careers of residual deviance (1966:92-3).

Upon inspection, it appears that propositions one, three, seven and nine offer the most ammunition to someone interested in countering Scheff's theory.

Scheff's initial hypothesis states that residual rule-breaking arises from diverse or random sources. I take this to mean that, if the existence of several *systematic* sources of residual rule-breaking can be shown, then the official application of a label would be viewed merely as a signpost in the eventual mental illness career.

This eventuality has come to pass with respect to schizophrenia,[1] as evidence favor-

*I would like to thank the editor and an anonymous reviewer for their helpful criticisms of an earlier version of this comment.

[1] In summarizing Linsky's findings, Scheff failed

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ing the existence of genetic abnormalities (cf. Gottesman and Shields, 1972) and various physiological defects (cf. Wyatt et al., 1971) has been introduced with increasing frequency and support. Further evidence for the superfluity of the label application is present in the often-cited Yarrow et al., (1955) study which noted that a pattern of stable mental deviance existed some time before an official label was applied. It was only when the wife could no longer interpret her husband's behavior in non-psychiatric terms that hospitalization was initiated.

Scheff objects to the conclusions of this study for two reasons. First, it is retrospective and deals only with a clinical population. While this is true, the mind boggles at the methodology that would be required to eliminate these problems. One would have to select a random sample of families, assign observers (unbiased, of course) to each, hope that over a period of time some would begin to exhibit repeated patterns of residual deviance (perhaps as a response to the unbiased observers), and then watch what happens over another extended period of time.

Until some bold and well-funded investigator takes up the challenge, I think we can be consoled by the fact that the conclusions of several other studies, although limited in the same respects as is Yarrow et al.'s, have supported their results. Sampson et al., (1962) report that husbands of schizophrenic women were able to deny their wives' increasingly bizarre behavior for up to several years before being convinced of the need for hospitalization. Similarly, Bakwin (1963) and Silver (1955:210-1) show the extent to which parents of schizophrenic children attempt to provide normal definitions to behavior which continually degenerates. Other examples of deviance denial in the face of progressively deteriorating behavior include Jackson's (1972) study of wives' reaction to alcoholic husbands and Davis' (1963) investigation of familial reactions to polio-victimized children.

Scheff also objects to Yarrow et al.'s conclusions because they are based on wives' reports rather than on observations of the "actual" situation. Although the study could have been improved by including the perceptions of other family members, the methodology was hardly capricious. The investigators interviewed thirty-three wives approximately ten times each over a period of six months (Clausen et al., 1955:9-10). As a reliability check, they also asked some wives to repeat their accounts of the pre-hospital situation which were initially related to the investigators some time before. It should also be noted that, of the other studies mentioned which support Yarrow et al., only Jackson (1972) relied solely on the perceptions of one family member.

Scheff's third proposition deals with the "transitory nature" of mental illness. He emphasizes that mental illness becomes stabilized due to the societal reaction ("the single most important factor [but not the only factor] in the stabilization of residual deviance"—Scheff, 1966:54). He goes on to deny that this means mental illness is merely "role-playing and pretence [sic]" (Scheff, 1966:54) but that he does intend to emphasize the "involuntary aspects of role-playing" (Scheff, 1966:56) in his model.

While even self-admitted schizophrenics claim to have engaged in role-playing to achieve desired ends (cf. Jefferson, 1964; Cecil, 1964), it becomes obvious upon inspecting the existing case studies of schizophrenic patients (cf. Kaplan, 1964) that schizophrenic symptomatology is something apart from role-playing—involuntary or otherwise. Further evidence on the likely independence of role-playing and schizophrenic symptomatology is found in Freeman and Simmons (1963:160) and Angrist et al. (1968:174-5).

Scheff's seventh proposition asserts that labelled deviants are punished when attempting to return to conventional roles due to the influence of stigma associated with the ex-mental patient status. A review of the literature dealing with stigma on former patients generally shows that this problem is not as severe as Scheff's proposition implies. Although Nunnally (1961:233) found that most people attach a stigma to the mentally ill, Cumming and Cumming (1965), Jones et al. (1963), Linn (1968) and Gove and Fain (1973) found that most ex-patients do not report being seriously concerned with the reactions of others. Further, Freeman and Simmons (1963:162-3) and Angrist et al. (1968:100, 176) believe that stigma may be

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to report one of Linsky's conclusions. To wit:

> Explanations based on differential community response do not appear to operate with regard to the psychoses .... The 'reaction' explanation would seem relevant chiefly for more marginal cases of mental deviance (Linsky, 1970a: 310).

Since many of the studies marshalled against labelling deal with schizophrenia (a psychotic disorder), I concur with Linsky to the extent that, if labelling is relevant, it is only applicable to less serious mental disabilities.

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more profitably viewed as a *consequence* rather than as a cause of career deviance.

Scheff's ninth proposition claims a causal link between labelling and careers of residual deviance. Upon testing this assertion, Robins (1966) concluded that children labelled by parents, schools and psychiatric clinics were no more deviant as adults than non-labelled children. Only in the case of children labelled by the courts did a significant difference emerge. It is also helpful to examine the inverse relationship—the degree to which career deviance can exist without labelling. It appears that some deviant pursuits are so attractive that people will engage in them regardless of the threat of a label. Examples include Brown (1965:169) on prostitution, Cressey (1953:114-38) on embezzlement, Lemert (1967:99-137) on systematic check forgers and Becker (1963:41-78) on regular marijuana users.

SCHEFF'S DEFENSE

Given the persuasive evidence favoring the developmental model of mental illness, a significant literature arguing against the importance of stigma and the problematic association between labelling and career deviance, I would expect Scheff to speak to at least some of these issues. Instead, all he offers are studies purporting to show that people are not treated "fairly" by official agencies. He cites four studies that provide "only very weak support" (Scheff, 1974:448) for labelling because of their inability to separate effects of mental illness from effects of powerlessness. To my way of thinking, studies that simultaneously support opposing positions (labelling and the medical model) in effect, support neither.

Among several "much stronger studies" Scheff (1974:449) notes that Wilde (1968) and Scheff (1964) show that commitment proceedings operate under the presumption of mental illness. The question is: so what? He provides no evidence to differentiate the developmental model of mental illness from the labelling model. We have nothing which shows that familial or community labelling brought the client to the commitment proceeding, as opposed to the development of a chronic disease which was denied as long as possible by significant others.[2] There is also no evidence to show that commitment forces the client to adopt the deviant identity—a critical issue in labelling theory.

A third "much stronger study" is provided by Greenley (1972) who concludes that psychiatrists are more likely to release patients to their families if these families want them back. This seems to be a logical move on the part of psychiatrists, and I commend them for it. Regardless, I cannot see where this conclusion has any relevance to Scheff's claim (1966:92-3) that "labelling is the single most important cause of careers of residual deviance," since everyone in Greenley's sample had already been labelled and presumably was already on his way toward a deviant career. Wenger and Fletcher (1969) are also billed as supporting labelling by finding a high association between the presence of legal counsel at the commitment hearing and the decision not to commit. This relationship held regardless of the authors' perceptions of the severity of the potential patients' condition. However, it is also true that those potential patients who were perceived as not meeting the criteria for commitment were more likely to obtain lawyers than were those considered to be borderline or those meeting the commitment requirements (Wenger and Fletcher, 1969:71). Thus, psychiatric conditions were not controlled for, as Scheff states (1974:449).

Scheff also cites studies by Linsky (1970a,b) and Rushing (1971) which show a relationship between powerlessness and commitment rates. I have three problems with this conclusion. First, Scheff claims that the ratio of involuntary to voluntary commitments controls for patient condition. I do not see how. Are we to assume that voluntary commitments are "sicker" than involuntary commitments because the voluntary ones have already internalized the deviant role while the involuntary ones have not? Or, are the involuntary patients sicker then the voluntary ones because the former are so far gone that they don't realize they are sick, while the latter group is at least in touch with reality to the extent that they realize the necessity of hospitalization? Second, as Gove and Howell

[2] The general thrust of Scheff's 1966 statement is that the label is applied only by officials at the time of commitment. However, in his 1974 statement, he mentions (p. 448) that labelling likely occurs much earlier—"even in the family or neighborhood." Taking this latter statement as the one he now favors, evidence relating to commitment proceedings cannot be used to argue for or against labelling since one does not know whether the prospective patient was brought to the proceeding to legitimize an informal label or as a last resort in the face of a developmental disorder.

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(1974:88) point out, the psychiatric perspective also predicts that lower class patients are more likely to be involuntarily committed than are middle or upper class patients because lower class folks view hospitalization in a less favorable light. Third, as Scheff reports (1974:450), Gove and Howell's (1974) conclusions are at odds with those of Linsky and Rushing, which leaves the issue unresolved.

Scheff goes on to mention two final studies which "provide still stronger support" (1974:450) for his position. Temerlin (1968) found (a) that psychiatrists are suggestible like everyone else and (b) that they believe it safer to diagnose illness in the face of ambiguity than to assume normality. After reading Rosenhan (1973), one can conclude:

(a) Hospitals assume that people seeking admission because they are hearing voices are sincere.
(b) Hospitals released these pseudo-patients within three weeks (on the average) after admission.
(c) Medical personnel all too familiar with the unpredictability of human behavior are reluctant to pronounce anyone as "cured."

CONCLUSION

In the introductory chapter of Scheff's 1966 statement, he tells us that labelling will not reject the medical model in its entirety, as his purpose is merely to provide an antithesis upon which a synthesizing model can be built. Yet the tone of his final proposition ("labelling is the single most important cause. . .") leads me to believe that he wishes labelling to replace, rather than coexist with, the medical model. The criticisms made here reflect his inability to respect the *limits* of sociology[3] (i.e., that it cannot account for disease entities), and are not directed toward the general proposition that social contingencies exist in the conferral and treatment of the sick role. Rather than offer a way to unite the strengths of labelling with those of the medical model, Scheff's 1974 statement merely attempts to defend the existence of the social reality while ignoring the question of its relative significance with respect to disease.

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[3] I would like to thank Harold Finestone for alerting me to the commonsensical but much neglected notion that sociology has limits.

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REPLY TO CHAUNCEY AND GOVE

I am pleased to reply to Chauncey's and Gove's lengthy comments, since they provide the opportunity to further spell out the meaning of some of my earlier statements. Chauncey's comments seem clearer to me, so I will reply first to them, and to Gove, where he makes similar points. I will then respond to some of Gove's additional points.

Chauncey begins by mistakenly attributing to me the notion that residual rule-breaking arises from diverse or random sources. This error leads him to a discussion of systematic sources, and to citing evidence regarding genetic and physiological defects. His argument does not seem relevant since the idea of random sources is his, not mine, and I have always accepted the validity of claims of genetic and physiological sources: "It has been demonstrated repeatedly that particular cases of mental disorder had their origin in genetic, bio-chemical or physiological conditions." (Scheff, 1966:40.)

In his reasoning concerning systematic sources, he seems to be attributing to me the intention of explaining all cases of stable madness by the labeling process; an intention that I have gone to great lengths to rule out: "There are, of course, conditions (such as psychiatric symptoms arising from paresis) which do fit the model of a progressively unfolding disease" (Scheff, 1966:52.) Chauncey suspects that I want to replace psychiatric theory with labeling theory because of my statement that the societal reaction is the most important single factor in the stabilization of residual rule-breaking. As I indicated many times in the book, I did not intend anything so grandiose. My reasoning was that

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